XIII. *Experiments to determine the Value of the British Association Unit of Resistance in Absolute Measure.*

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[PLATE 48.]

THE present paper relates to the same subject as that entitled "On the Determination of the Ohm in Absolute Measure," communicated to the Society by Dr. SCHUSTER and myself, and published in the Proceedings for April 12, 1881—referred to in the sequel as the former paper. The title has been altered to bring it into agreement with the resolutions of the Paris Electrical Congress, who decided that the ohm was to mean in future the absolute unit (10^9 C.G.S.), and not, as has usually been the intention, the unit issued by the Committee of the British Association, called for brevity the B.A. unit. Much that was said in the former paper applies equally to the present experiments, and will not in general be repeated, except for correction or additional emphasis.

The new apparatus (Plate 48) was constructed by Messrs. ELLIOTT on the same general plan as that employed by the original Committee, the principal difference being an enlargement of the linear dimensions in the ratio of about 3 : 2. The frame by which the revolving parts are supported is provided with insulating pieces to prevent the formation of induced electric currents, and more space is allowed than before between the frame and those parts of the ring which most nearly approach it during the revolution. It is supported on three levelling screws, and is clamped by bolts and nuts to the stone table upon which it stands. The ring is firmly fastened by nuts to two gun-metal pieces which penetrate it at the ends of the vertical diameter, and which form the shaft on which it rotates. The lower end of the bottom piece is rounded, and bears upon a plate of agate, on which the weight of the revolving parts is taken. A little above this comes the driving pulley (9 inches in diameter), and above this again the screw and nut by which the divided card is held. The top piece is hollow, forming a tube with an aperture of $1\frac{1}{4}$ inches, and is held by a well-fitting brass collar attached to the upper part of the frame. On this bearing the force is very small, so that the considerable relative velocity of the sliding surfaces has no ill effect. Notwithstanding its great weight, the ring ran very lightly, and the principal resistance to be overcome was that due to setting air in motion.

In the original apparatus the ring is very light, in fact scarcely strong enough to stand the forces to which it is subjected in winding on the wire. In order to avoid this defect, and also on account of its larger size, the new ring was made very massive. Cast solid, with lugs at the ends of what was to be in use the horizontal diameter, it was cut into two equal parts along a horizontal plane. The two parts were then insulated from one another by a layer of ebonite, and firmly joined together again at the lugs by bolts and nuts, after which the grooves, &c., were carefully turned. As it was intended to use two coils of wire in perpendicular planes, two rings were prepared. The smaller ring fitted into the larger, the end pieces passing through holes along the vertical diameters of both. But for a reason that will presently be given, only the larger ring was used in the present experiments.

In the spring of 1881 the larger ring was wound in Messrs. ELLIOTTS' shop under the superintendence of Dr. SCHUSTER and myself, and the necessary measurements were taken. On mounting the apparatus a few days later in the magnetical room of the Cavendish Laboratory, and making preliminary trials, we were annoyed by finding a very perceptible effect upon the suspended magnet even when the wire circuit was open. The currents thus indicated might have been due to a short circuit in the wire, or more probably (considering that the wire was triple covered, and that the winding had been carefully done) had their seat in the ring itself. Experiment showed that the insulation between the two parts of the ring, as well as between the wire and each part, was very good, so that no currents could travel round the entire circumference; but on consideration it appeared not unlikely that currents of sufficient intensity might be generated in those parts of the ring which lie nearest to the ebonite layer. The width of the ring (in the direction of its axis) was 4 inches, and the least thickness—that at the bottom of the grooves—about $\frac{3}{8}$ inch, so that the operative parts may be compared to four vertical plates $\frac{3}{8}$ inch thick, 4 inches broad, and (say) 6 inches high. In these plates currents will be developed during the rotation, whose plane is perpendicular to that of the currents in the wire.

The unwished for currents could doubtless have been much diminished by saw cuts in a vertical plane extending a few inches upwards and downwards from the insulating layer, but it appeared scarcely safe to assume that the ring would retain its shape under such treatment. It would have been wiser to have tried the effect of spinning the ring alone before winding on the wire, but we were off our guard from the fact that the old ring gave no perceptible disturbance.

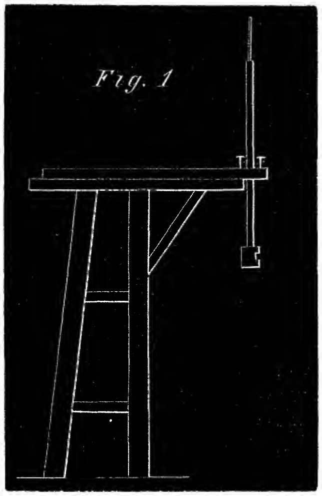
Theory having shown that these currents, if really formed in the manner supposed, could be satisfactorily allowed for, we decided to proceed with the experiment. At the worst, the differential effect between wire circuit closed and wire circuit open could only be in error by a quantity depending upon the square of the speed, and therefore capable of elimination upon the evidence of the spinnings themselves; while if the view were correct that the disturbing currents were principally in a plane perpendicular to that of the wire, even the correction for induction would not be much affected. A

special experiment, in which the ring (with wire circuit open) was oscillated backwards and forwards through a small angle in time with the natural vibrations of the magnet, allowed us to verify the plane of the currents. A marked effect was produced when the plane of the ring was east and west, but nothing could be detected with certainty when it was north and south—the opposite of what would happen with the wire circuit closed. After this, no doubt could remain but that most of the disturbance was due to currents in the ring, and subsequent spinnings after the removal of the wire have proved that no sensible part of it was caused by leakage through the silk insulation. The existence of this disturbance, however, so far modified our original plan as to induce us to omit the second ring as giving rise to too great a complication.

The suspended magnet was made of four pieces of steel attached to the edges of a cube of pith and of such length (about $\frac{1}{2}$ inch) as to be equivalent in their action to an infinitely small magnet at the centre of the cube. Before the pieces were put together the approximate equality of their magnetic moments was ascertained. The resultant moment was between six and seven times as great as that used in our former experiments. In virtue of the greater radius of the coil, this important advantage was obtained without undue increase of the correction for magnetic moment, which amounted to about ·004, only twice as great as before. The effect of mechanical disturbances, such as air currents, was still further reduced by diminishing the size of the mirror, particularly in its horizontal dimension. On both accounts the influence of air currents was probably lessened about 15 times, and, in fact, no marked disturbance was now caused by the proximity of a lamp to the magnet box.* In consequence of these changes, however, it was found necessary to introduce an inertia ring in order to bring the time of vibration up to the amount (about $5\frac{1}{2}$ seconds from rest to rest) necessary for convenient observation. The diameter of the ring was about $\frac{3}{4}$ inch, and the whole weight of the suspended parts was not too great to be borne easily by a single fibre of silk. A brass wire passing between the spokes of the ring prevented the needle from making a complete revolution.

The enlarged scale of the apparatus allowed us to introduce a great improvement into the arrangement of the case necessary for screening the suspended parts from the mechanical disturbance of the air caused by the revolution of the coil. A brass tube of an inch in diameter was not too large to pass freely through the hollow axis. At its lower extremity (fig. 1) it was provided with an outside screw, to which the magnet box was attached air-tight. By unscrewing the box, whose aperture was large enough to allow the inertia ring to pass, the suspended parts could be exposed to view, and by drawing up the brass tube they could be removed altogether, so as to allow the coil to be dismounted, without breaking the fibre. The upper end of the fibre was attached to a brass rod sliding in a socket at the upper end of the tube, by which the height of the magnet could readily be adjusted. The whole was supported on three screws

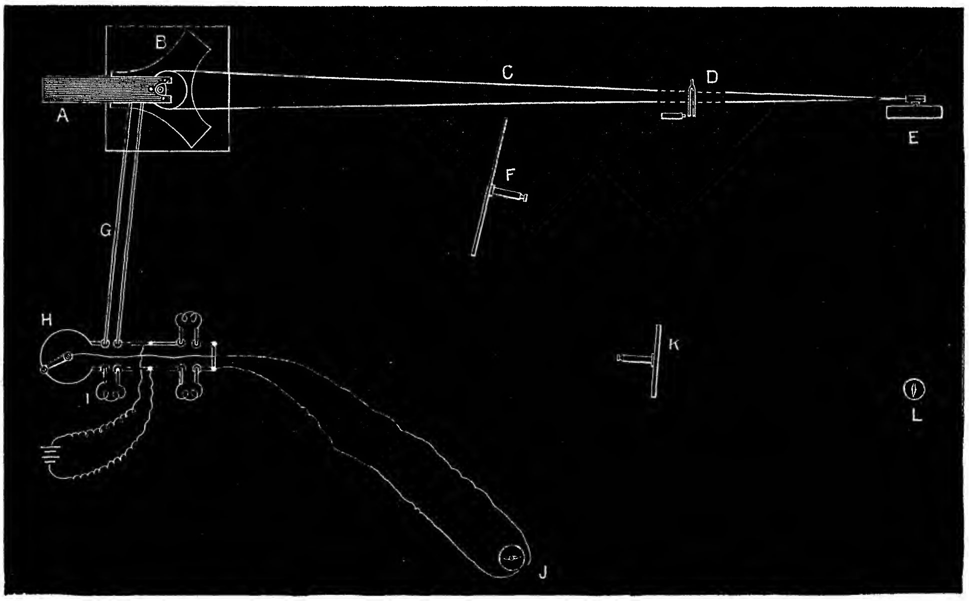
* See pp. 115–132 of the former paper.



passing through the corners of a brass triangle attached to the tube not far above the place where it emerged from the hollow axis. The points of the screws rested upon the same overhanging stand as in the former experiments (p. 113).*

The larger diameter of the tube made the system so rigid that no mechanical disturbance of the kind formerly met with was to be detected at the highest speed to

* June, 1882. The general disposition of the apparatus is shown in fig. 2.



A. Stand for suspended parts.
B. Frame of revolving coil.
C. Driving cord.
D. Electro-magnetic fork and telescope.
E. Water engine.
F. Principal telescope and scale.
G. Copper connecting bars.
H. FLEMING's bridge.
I. Platinum-silver standard.
J. Bridge galvanometer.
K. Telescope and scale of auxiliary magnetometer.
L. Auxiliary magnetometer needle and mirror.

which we could drive the coil. Even a tap with the finger-nail upon the magnet-box produced but a small disturbance.

No change was required in the arrangements for regulating and determining the speed of the coil, which worked, if possible, more perfectly than before, in consequence of the greater inertia of the revolving parts. The divided card was, however, on an enlarged scale, and the numbers of the teeth in the various circles were so arranged that each circle was available for a distinct pair of speeds according as it was observed through the slits in the plates carried by the electric fork or over the top of the upper plate. The speeds actually used corresponded to 80, 60, 45, 35, and 30 teeth, seen through the slits, *i.e.*, about 127 times per second.

The greater resistance of the copper coil (23 instead of 4·6) rendered necessary a modification in the method of making the comparisons with the standard. The whole value of the divided platinum-iridium wire on FLEMING's bridge being only $\frac{1}{20}$ ohm, a change of temperature in the copper of not much more than a degree would exhaust the range of the instrument. To meet this difficulty it was only necessary to add resistances to the copper circuit so as to compensate approximately the temperature variations, for it is evident that it can make no difference whether the change of resistance of the entire revolving circuit is due to a rise of temperature, or to the insertion of an additional piece. The platinum-silver standard was therefore prepared so as to have a resistance (about 24 ohms) greater than any which we were likely to meet with in the copper, and the additional pieces were relied upon to bring the total within distance. As at first arranged, the additional resistance was inserted at the mercury cups, instead of a contact piece of no appreciable resistance. During the comparison with the standard it was transferred to another part of the circuit.

In the course of May, 1881, a complete series of spinnings were taken, the arrangements and adjustments being (except as above-mentioned) in all respects the same as with the old apparatus. Five different speeds were used, and each of them on three different evenings. The work of observing was also distributed as before, Dr. SCHUSTER taking the readings of the principal magnetometer, and Mrs. SIDGWICK the simultaneous readings of the auxiliary magnetometer, while I observed the divided card and regulated the speed. At each speed on each evening four readings were taken with wire circuit closed, two with positive and two with negative rotation, and in like manner four readings were taken with the wire circuit open. Observations on the zero with the coil at rest were for the most part dispensed with, as it was thought that the time could be better employed otherwise; in fact, the mean of the two not very different positions of equilibrium obtained with positive and negative rotation when the wire circuit was open, gives all that is wanted in this respect. In the actual reductions we only require the *difference* of readings with positive and negative rotations.

It was hoped that these observations would have been sufficient, but on the introduction by Dr. SCHUSTER of the various corrections for temperature, for the beats between

the two forks, and for the outstanding bridge-wire divisions, the necessity for which disguises the significance of the numbers first obtained, it was found that the agreement of the results corresponding to a given speed was by no means so good as we had expected in view of the precautions taken and the accuracy of the readings. What was worse, there was evidence of a decided progression, as if the absolute resistance of the standard had gradually diminished during the time occupied by the spinnings.

It is not impossible that there really was some change in the standard which had then been newly prepared; but the discrepancies were not, as according to this view they ought to have been, proportional to the speeds of rotation. I am inclined rather to attribute them to shiftings of the paper scales. The principal magnetometer scale was composed of three lengths of 50 centims. each, cemented with indiarubber to a strip of deal. The compound scale thus formed was examined by Dr. SCHUSTER in March, 1881. Between the graduations of the first and of the middle piece there was a gap of about $\frac{1}{4}$ millim., and another of nearly the same magnitude between the middle and the third piece. When I re-examined the scale in July, the gap at 500 divisions had increased to $\frac{9}{10}$ millim., and that at 1000 to $\frac{1}{2}$ millim. Curiously enough, there were no observable errors in the equality of the divisions of the three parts taken separately; but the changes above-mentioned are sufficient to throw considerable doubts upon the value of the first series of spinnings. They have, however, been reduced by Dr. SCHUSTER, and the result is given below for the sake of comparison.

To be free for the future from uncertainties of this kind, I replaced the paper scale by a long glass thermometer tube by CASELLA, graduated into millimetres. The divisions were fine and accurately placed, but the imperfect straightness of the tube has rendered necessary certain small corrections in the final results. Probably a straight strip of flat opal would have been an improvement.

The second series of spinnings was made in August, 1881, and this, it was fondly hoped, would be final. To guard against possible change in the platinum-silver coil a careful comparison with the standard units was previously instituted by Mrs. SIDGWICK, of which the details are given later. As we had unfortunately lost the advantage of Dr. SCHUSTER'S assistance, the observations at the principal magnetometer devolved upon Mrs. SIDGWICK. The much easier post at the auxiliary magnetometer was usually occupied by Lady RAYLEIGH; occasional assistance has been rendered by Mr. A. MALLOCK and by Mr. J. J. THOMSON.

In the conduct of the second series one or two minor changes were introduced. In order to know the temperature of the standard tuning-fork more accurately, a thermometer was placed between its prongs and read at the same time as the number of beats was taken. The insertion of the small resistances necessary to bring the copper coil within range of the standard was also arranged in a different manner. Some trouble had been experienced in getting a sufficiently good fit between the contact pieces used in the first series and the mercury cups. It is necessary that the stout copper terminals should press down closely upon the bottoms of the cups, and also that the mercury

should not be liable to escape at high speeds from the effect of centrifugal force. Bits of indiarubber tubing were placed round the copper legs, by which a fair fit with the sides of the cups was effected; but I thought that it would be an improvement to revert to a single contact piece for the mercury cups of no sensible resistance, whose fit could be carefully adjusted, and to insert the extra resistances at the connexion of the other (outer) ends of the component coils. For this purpose binding screws were employed, pressing firmly together the flat copper terminals of the copper wire and of the German-silver resistance pieces. It is almost unnecessary to say that these short lengths of German-silver wire were doubled upon themselves before being coiled, and that the pieces were not touched between a spinning and the associated resistance comparisons. Used in this way the screwed up contacts seemed unobjectionable, even though the surfaces were not amalgamated.

On each night and for each speed a set of twelve spinnings was made, six with wire circuit open, and six with wire circuit closed. It was usual to take, first, two of the former (one with positive and one with negative rotation); secondly, to compare the resistances of the revolving circuit and the standard; thirdly, after inserting the contact piece and adjusting the indiarubber strap by which it was held down, to make the six closed contact spinnings; fourthly, to compare the resistances again; and lastly, to complete the open contact readings. Each spinning, it will be understood, involved the reading of several elongations (about six for the open contact and ten for the closed), from which the position of equilibrium was deduced.

Table II. (p. 691) gives all the results of the second series, except one for 35 teeth on August 27th, which was rejected on the ground that it exhibited such large *internal* discrepancies, as to force us to the conclusion that the contact piece had been inserted improperly. It will be seen that the agreement is good except on August 29th, in which case the deflections are as much as four or five tenths of a millimetre too small. These discrepancies, though not very important in themselves, gave me a good deal of anxiety, as they were much too large to be attributed to mere errors of reading, and seemed to indicate a source of disturbance against which we were not on our guard.

The least unlikely explanations seemed to be (1) a change in the distance of the mirror from the scale, which unfortunately had not been remeasured at the close of the spinnings, though this would require to reach 3 millims.; (2) imperfect action of the contact piece from displacement of mercury or otherwise; (3) a change of level in the axis of rotation. The anomalous result of August 27th seemed to favour (2), while on behalf of (1) it must be said that the stand of the telescope and scale as well as the support for the suspended parts of the principal magnetometer were of wood. It was just conceivable that under the influence of heat or moisture some bending might have occurred.

On my return to Cambridge in October we proceeded to investigate these questions with the closest attention. As repeated direct measurements of the distance of the mirror and scale were inconvenient, measuring rods (like beam compasses) were provided

to check the relative positions of the telescope stand and of the upper end of the suspending fibre with regard to fixed points on the walls of the room. But no changes comparable with 3 millims. were detected, even under much greater provocation than could have existed during the August spinnings. The next step was to examine the action of the contact piece. For this purpose the coil was balanced against the standard as usual, except that the contact piece was inserted and connexion with the bridge made at the other ends of the double coil. It was presently found that the resistance *did* depend upon the manner in which the contact piece was pressed, and that to an extent sufficient to account for the August discrepancies. Eventually it was discovered that one of the legs of the contact piece, which by a mistake had been merely rivetted and not soldered in, was shaky.

After this there could be no reasonable doubt that the faulty contact piece was the cause of our troubles. In all probability the leg became loose on the 27th, in which case the earlier results would be correct. Moreover, the final means are not very different, whether the spinnings of August 29th are retained or not. This being the case, we might perhaps have been content to let the matter rest here ; but in view of the importance of the determination, and the desirability as far as possible of convincing others as well as ourselves, we thought that it would be more satisfactory to make a third and completely independent series of spinnings.

In this series the faulty composite contact piece was replaced by a horse shoe of continuous copper, and a check was instituted upon the distance between mirror and scale. The opportunity was also taken to make a minor improvement in connexion with the auxiliary magnetometer. The somewhat unsteady table on which the telescope and scale had stood was replaced by one of stone, and the arrangements for illumination were improved by throwing an image of a gas flame on the part of the scale under observation. The same number of readings were made as in the second series, but we found it more expeditious to take the six open contact spinnings together. At the beginning of the evening it was desirable to commence with these open contact spinnings in order to give more time for the coil to acquire the temperature of the room, which always rose somewhat, although the lamps and gas were lit a couple of hours beforehand. Later in the evening we sometimes took the closed contact readings for two speeds consecutively, in order that the intermediate resistance comparison might serve for both. In other respects the arrangements were unaltered.

Full details of the observations and reductions are given below. It will be sufficient here to mention that the maximum discrepancy between any two deflections at the same speed amounts only to $\frac{7}{100}$ of a millimetre, so that the agreement on different nights is more perfect than could have reasonably been expected. At the lowest speed the above-mentioned discrepancy is less than one part in 3000, and at the highest speed less than one part in 6000. No spinnings in the third series were rejected, except on one or two occasions when it appeared *at the time of observation*, from the behaviour of the auxiliary magnetometer, that there was too much earth

disturbance. The spinnings were then suspended, and the observations already obtained were not reduced.

At the close of the spinnings, Mrs. SIDGWICK made a further comparison of our platinum-silver coil with the standard units.

The value arrived at for the B.A. unit (·9865 ohm) differs nearly three parts in a thousand from that which we obtained with the original apparatus. This difference is not very great, and may possibly be accounted for by errors in the measurement of the coil (see p. 114 of former paper). If a coil be imperfectly wound, the mean radius, as determined by a tape, is liable to be too great. At any rate, this discrepancy sinks into insignificance in comparison with that which exists between either of these determinations and that of Professor KOHLRAUSCH,* according to whom the B.A. unit would be as much as 1·0196 ohms. With respect to the method employed by KOHLRAUSCH, I agree with ROWLAND† in thinking it difficult, and unlikely to give the highest accuracy; but how in the hands of a skilful experimenter it could lead to a result 3 per cent. in error, is difficult to understand. The only suggestion I have to make is that possibly sufficient care was not taken in levelling the earth-inductor. Although estimates are given of the probable errors due to uncertainties in the various data, nothing is said upon this subject. In consequence, however, of the occurrence of the horizontal intensity as a square in the final formula, in conjunction with the largeness of the angle of dip, the method is especially sensitive to a maladjustment of this kind. I calculate that a deviation of the axis of rotation from the vertical through 21′ in the plane of the meridian, would alter the final result by 3 per cent.‡

According to ROWLAND'S determination, the value of the B.A. unit is ·9912 ohm. The method consists essentially in comparing the integral current in a secondary circuit, due to the reversal of the battery in a primary circuit, with the magnitude of the primary current itself. The determination of the secondary current involves the use of a ballistic galvanometer, whose damping is small, and whose time of vibration can be ascertained with full accuracy; and it is here, I think, that the weakest point in the method is to be found. The logarithmic decrement is obtained by observation of a long series of vibrations, and it is assumed that the value so arrived at is applicable to the correction of the observed throw. I am not aware whether the origin of damping in galvanometers has ever been fully investigated, but the effect is usually supposed to be represented by a term in the differential equation of motion proportional to the momentary velocity. This mode of representation is no doubt applicable to that part of the damping which depends upon the induction of currents in the galvanometer coil, under the influence of the swinging magnet. If this were all, a correction for damping would be accurately effected on the basis of a determination of the logarithmic decrement, made with the galvanometer circuit closed in the same

* POGG. Ann., Ergänzungband VI. Phil. Mag., April, 1874.

† American Journal, April, 1878.

‡ See p. 684.

manner as when the throw is taken. In all galvanometers, however, a very sensible damping remains in operation even when the circuit is open, of which the greatest part is doubtless due to aerial viscosity; and it is certain that the retarding force arising from viscosity is not simply proportional to the velocity at the moment, without regard to the state of things immediately preceding.

In particular, the force acting upon the suspended parts as they start suddenly from rest in the observation of the throw, must be immensely greater than in subsequent passages through the position of equilibrium, when the vibrations have assumed their ultimate character. I calculate that in the first quarter vibration (*i.e.*, from the position of equilibrium to the first elongation) of a disc vibrating in its own plane and started impulsively from rest, the loss of energy from aerial viscosity would be 1·373 times that undergone in subsequent motion between the same phases. From this it might at first appear that in this ideal case the logarithmic decrement observed in the usual manner would need to be increased by more than a third part in order to make it applicable to the correction of a throw from rest; but in order to carry out this view consistently we should have to employ in the formula the time in which the needle would vibrate if the aerial forces were non-existent instead of the actually observed time of vibration. Now since the action of viscosity is to increase the time of vibration, the second effect is antagonistic to the first, so that probably the error arising from the complete neglect of these considerations is very small.

There is another point in which it appears to me that the theory of the ballistic galvanometer is incomplete. It is assumed that the magnetism of the needle in the direction of its axis is the same at the moment of the impulse as during regular vibrations. Can we be sure of this? The impulse is due to a momentary but very intense magnetic force in the perpendicular direction, and it seems not impossible that there may be in consequence a temporary loss of magnetism along the axis. If this were so, the actual impulse and subsequent elongation would be less than is supposed in the calculation, and too high a value would be obtained of the resistance of the secondary circuit in absolute measure. In making these remarks I desire merely to elicit discussion, and not to imply that ROWLAND's value is certainly four parts in a thousand too high.

Determinations of the absolute unit have been made also by H. WEBER,* whose results indicate that the B.A. unit is substantially correct. In the absence of sufficient detail it is difficult to compare this determination with others, so as to assign their relative weights.

The value of the B.A. unit in absolute measure is involved in the two series of experiments executed by JOULE on the mechanical equivalent of heat.† The result from the agitation of water is 24868, while that derived from the passage of a known absolute current through a resistance compared with the B.A. unit was 25187. The

* Phil. Mag., Jan., Feb., March, 1878.

† Phil. Trans., Part II., 1878. Brit. Ass. Rep., 1867; Reprint, p. 175.

latter result is on the supposition that the B.A. unit is really 10^9 C.G.S. If we inquire what value of the B.A. unit will reconcile the two results, we find—

$$1 \text{ B.A. unit} = \cdot 9873 \text{ ohm},$$

in very close agreement with the measurement described in the present paper. It should be remarked that in the comparison of the two thermal results some of the principal causes of error are eliminated; and it is not improbable that an experiment in which heat should be simultaneously developed in one calorimeter by friction, and in a second similar calorimeter by electric currents, would lead to a very accurate determination of resistance, more especially if care were taken so to adjust matters that the rise of temperature in the two vessels was nearly the same, and a watch were kept upon the resistance of the wire while the development of heat was in progress.

[*June*, 1882.—Since this paper was sent to the Society, Mr. GLAZEBROOK has worked out the results of a determination of the B.A. unit in absolute measure by a method not essentially different from that adopted by ROWLAND. The final number is practically identical with that of the present paper; and the agreement tends to show that the difference between ourselves and ROWLAND is not to be attributed to the use of a ballistic galvanometer.

Reference should have been made to the results of LORENTZ.* He finds as the value of the mercury unit *defined* by SIEMENS

$$1 \text{ mercury unit} = \cdot 9337 \ \frac{\text{earth quadrant}}{\text{second}}$$

The corresponding number calculated from the results of the present paper with use of the value of the specific resistance of mercury lately found (Proc. Roy. Soc., May 4, 1882) is ·9413. If we invert the calculation, we find that according to LORENTZ the value of the B.A. unit would be ·9786 absolute measure. The method of LORENTZ is ingenious, and apparently capable with good apparatus of giving a result to much within 1 per cent. Mrs. SIDGWICK and myself are at present making a trial of it.]

It will be desirable here to consider briefly some of the criticisms of KOHLRAUSCH and ROWLAND upon the method of the original British Association Committee, which has been adopted in the present investigation without fundamental alteration. The difficulty, remarked upon by KOHLRAUSCH, of obtaining a rapid and uniform rotation, has not been found serious, and I believe that no appreciable error can be due either to irregularity of rotation or to faulty determination of its rapidity. It has also been brought as an objection to the method that a correction is necessary on account of the magnetic influence of the suspended magnet upon the revolving circuit. The theory of this action is, however, perfectly simple, and the application of the correction requires only a knowledge of the ratio of the magnetic moment to the earth's

* POGG. Ann., 1873.

horizontal force. If the magnetic moment is very small, the correction is unimportant; if larger, it can on that very account be determined with the greater ease and accuracy. It is probable that in the original experiments too feeble a magnetic moment was used, and that in consequence the suspended parts were too easily disturbed by non-magnetic causes; but this might have been remedied without increasing objectionably the correction in question. At any rate the larger coil of the new apparatus allows the use of any reasonable magnetic moment.

Perhaps the least advantageous feature in the method is the necessity for creating a violent aerial disturbance in the immediate neighbourhood of a delicately suspended magnet and mirror. If, however, any deflection occurs in this way, very little error can remain when the open contact effect is subtracted from the closed contact effect. The difficulty of avoiding a sensible deflection, due to currents in the ring, when the wire circuit is open, is connected with a special advantage—*i.e.*, the possibility of assuring ourselves that there is no leakage from turn to turn of the coil. In the method followed by ROWLAND, for instance, such a leakage would lead to error, and could not be submitted to any direct test.

The correction for self-induction cannot be made very small without a disadvantageous reduction of the whole angular deflection; but so far as the wire is concerned it can be calculated *à priori*, or determined by independent experiment, with the necessary accuracy. There is reason, however, to think that the best method of treatment is to determine this correction from the spinnings themselves, combining the results of widely different speeds so as to obtain what would have been observed at a small speed. At small speeds it is certain that all effects of self-induction and of mutual induction between the wire circuit and other circuits in the ring will disappear.

Measurements of coil.

The mean radius of the coil, being the fundamental linear measurement of the investigation, must be found with full accuracy. There has been some difference of opinion as to the best method of effecting this. The greatest accuracy is probably attained by the use of the cathetometer. The measurement of the circumference of every layer by a steel tape has the advantage that the subject of measurement is three times as large, and is much less troublesome. The disadvantage is that if a layer be not quite even, there is danger of measuring the maximum rather than the mean outside circumference. In the present investigation the coil was so large that the tape could be employed without fear.*

Each of the component coils marked A and B had $18 \times 16 = 288$ windings, but in

* The original Committee also employed the tape method. Their measurement of the length of the wire when unwound was not in order to find the mean radius, as SIEMENS and KOHLRAUSCH suppose, but to verify the number of turns.

consequence of variations in the thickness of the triple silk covering, there was a difficulty in getting exactly 18 turns into each layer. In the eleventh layer of A it was necessary to be content with 17 turns, and to place an extra turn on the outside, so as to form the commencement of a seventeenth layer—a circumstance which of course was taken into account in calculating the mean. The number thus arrived at, after correction for the thickness of tape, is the mean *outside* circumference. What we require is the mean circumference of the axis of the wire; it may be derived from the first by subtraction of half the difference between the tape readings for the first layer, and for the bottom of the gun-metal groove.

The results obtained by Dr. SCHUSTER and myself when the coils were wound are:

	Coil A.	Coil B.
Mean of readings in millims.	1489·3	1487·5
Correction for tape	·6	·6
Mean outside circumference	1488·7	1486·9
Correction for thickness of wire . .	3·4	3·4
Mean circumference	1485·3	1483·5
Mean radius	236·39	236·11
Mean circumference of A and B . . .	1484·4	
Mean radius of A and B (a)	236·25	
Axial dimension of section in millims. .	19·9	19·9
Radial	15·9	15·4
Distance of mean planes ($2b'$)	65·95	

Two or three readings were taken of the circumference of every layer, and to prevent mistakes in the number of turns, the plan described by MAXWELL,* of simultaneously winding string on wooden rods, was followed. Without some such device, there is great risk of confusion.

In estimating the degree of accuracy obtainable, we must remember that the circumference of each layer is measured before the outer layers are wound on; any change produced by the pressure of these outer layers is a source of error. We had already observed a tendency in the measurements to be less during the unwinding of a coil than during the winding, and we fully intended to remeasure the coil after the spinnings were completed. This was done on December 6, 1881, by Mrs. SIDGWICK and myself. As we expected, somewhat smaller readings (by about $\frac{3}{4}$ millim.) were obtained for the circumference of the middle layers. The results were:

* 'Electricity and Magnetism,' II., § 708.

	Coil A.	Coil B.
Mean radius	236·31	236·02
Mean of both	236·16	

or nearly one part in 2000 less than before. Of the two values, it would appear that the latter is more likely to represent the actual condition of the coil during the spinnings, and is therefore entitled to greater weight. If we give weights in the proportion of two to one, we get

Mean radius=23·619 centims.*

Calculation of GK.

We have

$$\mathrm{GK}=2\pi^2n^2a\sin^3\alpha\left\{1+\tfrac{1}{6}\frac{c^2}{a^2}+\tfrac{5}{8}\frac{b^2-c^2}{a^2}\sin^2\alpha\cos^2\alpha-\tfrac{1}{8}\frac{b^2}{a^2}\sin^2\alpha\right\}$$

in which

a	=mean radius	=23·625	(1st measurement)
b	=axial dimension of section	= 1·990	
c	=radial dimension of section	= 1·565	
n	=total number of turns	= 576	
$2b'$	=distance of mean planes	= 6·595	

$$\sin\alpha=a\div\sqrt{(a^2+b'^2)}$$

From these data we find

$$\begin{aligned}\log 2\pi^2n^2 &= 6\cdot81617\\ \log a &= 1\cdot37337\\ \log\sin^3\alpha &= \bar{1}\cdot98744\\ \log\{\ldots\} &= \bar{1}\cdot99995\\ \hline \log \mathrm{GK} &= 8\cdot17693\end{aligned}$$

But if we substitute the adopted value of a, *i.e.*, 23·619 centims., we have by subtraction of ·00011

$$\log\mathrm{GK}=8\cdot17682$$

Calculation of L.

We may write

$$\mathrm{L}=16^2\times18^2\,(\mathrm{L}_1+\mathrm{L}_2+2\mathrm{M}),$$

where L_1, L_2 are the coefficients of self-induction of the two parts, and M the

* [August, 1882. At the time of use the tape was compared with a measuring rod, which again has been compared with a standard metre verified by the Standards Department of the Board of Trade. For the purposes of this investigation the differences observed are altogether negligible. I may add that the clock with which the standard tuning-fork was compared (see p. 137 of former paper) was rated from astronomical observations.]

coefficient of mutual induction without regard to the number of turns. L_1 and L_2 may be calculated from the formula

$$L=4\pi a\left[\log_e\frac{8a}{r}+\tfrac{1}{12}-\tfrac{4}{3}(\theta-\tfrac{1}{4}\pi)\cot 2\theta-\tfrac{1}{3}\pi\operatorname{cosec}2\theta-\tfrac{1}{6}\cot^2\theta\log_e\cos\theta-\tfrac{1}{6}\tan^2\theta\log_e\sin\theta\right]$$

in which r is the diagonal of the section, and θ the angle between it and the plane of the coil. With this formula and with the dimensions as measured when the coil was wound, we get

$$L_1 \text{ (for A) } = 1029\cdot3 \text{ centims.}$$
$$L_2 \text{ (for B) } = 1031\cdot9 \text{ centims.}$$

It would not be difficult to calculate an approximate correction for the curvature of the coil, but this is scarcely necessary. (See p. 119 of former paper.) Adding the above, we have

$$L_1+L_2=2061\cdot2 \text{ centims.}$$

The value of M was found from the tables given as Appendix I. to § 706 of the new edition of MAXWELL's 'Electricity.' If we suppose each coil condensed into the centre of its section, we find $M=4\pi\times33\cdot061$. A more exact calculation by the formula of interpolation explained in Appendix II. gives $M=4\pi\times33\cdot140$, so that

$$2M=832\cdot88 \text{ centims.}$$

The final result is accordingly

$$L=16^2\times18^2\times2894\cdot1=2\cdot4004\times10^8 \text{ centims.}$$

These calculations of the coefficients of induction have been made independently by Mr. NIVEN and myself, and are so far reliable; but we must not forget that the accuracy of the result depends upon the accuracy of the data, and that in the present case the diagonal of the section (r) on which the most important part of L depends is an element subject to considerable relative uncertainty. It is probable that the effective axial dimensions of the section is somewhat less than the width of the groove, and therefore that the real value of L may be a little greater than would appear from the preceding calculation.

Theory of the ring currents.

If the circuits are conjugate, the currents in the wire and in the ring are formed in complete independence of one another, a circumstance which simplifies the theory very materially. In the same notation as was used in the former paper (p. 105), and with dashed letters for the ring circuit, we have as the equation determining the angle of deflection (ϕ) when the wire circuit is closed,

$$\tan\phi+\tau\frac{\phi}{\cos\phi}=\frac{\frac{1}{2}\mathrm{GK}\omega}{\mathrm{R}^2+\mathrm{L}^2\omega^2}\{\mathrm{R}+\mathrm{L}\omega\tan\phi+\mathrm{R}\tan\mu\sec\phi\}$$
$$+\frac{\frac{1}{2}\mathrm{G'K'}\omega}{\mathrm{R'}^2+\mathrm{L'}^2\omega^2}\{\mathrm{R'}+\mathrm{L'}\omega\tan\phi+\mathrm{R'}\tan\mu\sec\phi\}$$

When the wire circuit is open the equation determining the angle of deflection (ϕ_0) is

$$\tan\phi_0+\tau\frac{\phi_0}{\cos\phi_0}=\frac{\frac{1}{2}\mathrm{G'K'}\omega}{\mathrm{R'}^2+\mathrm{L'}^2\omega^2}\{\mathrm{R'}+\mathrm{L'}\omega\tan\phi_0+\mathrm{R'}\tan\mu\sec\phi_0\}$$

Since τ is an extremely small quantity it is unnecessary to keep up the distinction between $\tau\phi/\cos\phi$ and $\tau\tan\phi$. By subtraction

$$(1+\tau)(\tan\phi-\tan\phi_0)$$
$$=\frac{\frac{1}{2}\mathrm{GK}\omega}{\mathrm{R}^2+\mathrm{L}^2\omega^2}\{\mathrm{R}+\mathrm{L}\omega\tan\phi+\mathrm{R}\tan\mu\sec\phi\}$$
$$+\frac{\frac{1}{2}\mathrm{G'K'}\omega}{\mathrm{R'}^2+\mathrm{L'}^2\omega^2}\{\mathrm{L'}\omega(\tan\phi-\tan\phi_0)+\mathrm{R'}\tan\mu\,(\sec\phi-\sec\phi_0)\}$$

The last term is small, and we may neglect ($\sec\phi-\sec\phi_0$) in combination with $\mathrm{R'}\tan\mu$.

Moreover

$$\frac{\frac{1}{2}\mathrm{G'K'}\omega}{\mathrm{R'}^2+\mathrm{L'}^2\omega^2}=\frac{(1+\tau)\tan\phi_0}{\mathrm{R'}+\mathrm{L'}\omega\tan\phi_0}$$

so that

$$(1+\tau)(\tan\phi-\tan\phi_0)=\frac{\frac{1}{2}\mathrm{GK}\omega}{\mathrm{R}^2+\mathrm{L}^2\omega^2}\{\mathrm{R}+\mathrm{L}\omega\tan\phi+\mathrm{R}\tan\mu\sec\phi\}$$
$$+(1+\tau)(\tan\phi-\tan\phi_0)\frac{\mathrm{L'}\omega\tan\phi_0}{\mathrm{R'}+\mathrm{L'}\omega\tan\phi_0}$$

If now we write (GK) for $\mathrm{GK}/(1+\tau)$, we get

$$\tan\phi-\tan\phi_0=\frac{\frac{1}{2}(\mathrm{GK})\omega}{\mathrm{R}^2+\mathrm{L}^2\omega^2}\{\mathrm{R}+\mathrm{L}\omega\tan\phi+\mathrm{R}\tan\mu\sec\phi\}\{1+\frac{\mathrm{L'}\omega}{\mathrm{R'}}\tan\phi_0\}$$

The effect of L′ would therefore be to *increase* disproportionately the deflections at high speeds, *i.e.*, contrary to the effect of L. It appears, however, that in these experiments it could not have been sensible. At the highest speed $\tan\phi_0$ was about $\frac{1}{670}$, and ω about 26 per second, so that $\omega\tan\phi_0$ would be about $\frac{1}{26}$. The value of L′/R′ is difficult to estimate with any accuracy. But the value of L/R for the wire circuit is about ·01 second, and that for the ring circuit must be much less, so that the terms involving L′ may safely be omitted.

The quadratic in R then becomes

$$\mathrm{R}^2-\mathrm{R}\frac{\frac{1}{2}(\mathrm{GK})\omega(1+\tan\mu\sec\phi)}{\tan\phi-\tan\phi_0}+\mathrm{L}^2\omega^2-\tfrac{1}{2}(\mathrm{GK})\mathrm{L}\omega^2\frac{\tan\phi}{\tan\phi-\tan\phi_0}=0$$

whence

$$R=\frac{\frac{1}{2}(GK)\omega}{\tan\phi-\tan\phi_0}\left[\tfrac{1}{2}(1+\tan\mu\sec\phi)+\surd\{\tfrac{1}{4}(1+\tan\mu\sec\phi)^2-U(\tan\phi-\tan\phi_0)^2\}\right]$$

where

$$U=\frac{2L}{(GK)}\left\{\frac{2L}{(GK)}-\frac{\tan\phi}{\tan\phi-\tan\phi_0}\right\}$$

L *by direct experiment.**

Although the calculated value of L was the result of two independent computations, I considered that it would be satisfactory still further to verify it by an experiment with WHEATSTONE'S balance. The statement of this method and the final formula, as given on p. 116 of the former paper, being approximate only, it will be convenient here to repeat them with the necessary corrections.

The four resistances in the balance are two equal resistances (10 units each), that of the copper coil P, and a fourth resistance Q (nearly equal to P) taken from resistance boxes, of which P is the only one associated with sensible self-induction. When P and Q are equal, there is no permanent current through the galvanometer; but if the galvanometer circuit be first closed and then the battery current be made, broken, or reversed, the needle receives an impulse, whose magnitude depends upon L.

If x denote the change of current in the branch P, the action of self-induction is the same as that of an electromotive impulse in that branch of magnitude Lx, and the effect upon the galvanometer is that due to this electromotive impulse acting independently of the electromotive force in the battery branch.

In order now to get a second quantity with which to compare the induction throw, the resistance balance is upset in a known manner. If while Q remains unaltered, P be increased to $P+\delta P$, there is a steady current through the galvanometer, which we may regard as due to an electromotive force $\delta P.x'$ in the branch $P+\delta P$, x' being the current through the branch. If θ be the deflection of the needle under the action of the steady current, α the angular throw, and T the time of swing from rest to rest, we have by the theory of the ballistic galvanometer as the ratio of the instantaneous to the steady electromotive force

$$\frac{T}{\pi}\frac{2\sin\frac{1}{2}\alpha}{\tan\theta},$$

subject to a correction for damping; so that this expression represents the ratio of $Lx:\delta P.x'$. If the induction throw be due to the make or break of the battery circuit, x represents simply the current in the branch P. In the case where the battery

* In consequence of the necessity which ultimately appeared of introducing an arbitrary correction proportional to the square of the speed of rotation, the result of the present section does not influence the final number expressing the B.A. unit in absolute measure. The method, however, is of some interest, and (it is believed) has not been carried out before with the precautions necessary to secure a satisfactory result.

current is reversed, we may write $2Lx$ for Lx, understanding by x the same as before. As this method was the one actually adopted, we will write the result in the appropriate form

$$\frac{L.x}{\delta P.x'} = \frac{T}{\pi} \frac{\sin \frac{1}{2}\alpha}{\tan \theta}.$$

In the formula as originally given by MAXWELL, and as stated in the former paper, the distinction between x and x' (the currents before and after the resistance balance is upset) was neglected. This step is legitimate if δP be taken small enough, to which course however there are experimental objections. In order that $\tan \theta$ might be of suitable magnitude, it was found necessary to make the ratio of $\delta P : P$ equal to about $\frac{1}{300}$, a fraction too large to be neglected.

In carrying out the experiment it was found more convenient to insert the additional resistance in the branch Q, leaving P unaltered. By the symmetry of the arrangement it is evident that this alteration is immaterial, and that we may take the formula in the form

$$\frac{L}{Q} = \frac{L}{P} = \frac{\delta Q.x'}{Q.x} \frac{T}{\pi} \frac{\sin \frac{1}{2}\alpha}{\tan \theta},$$

x being the current in the branch Q when the resistance balance is perfect, x' the diminished current when the additional resistance δQ is inserted.

The principal difficulties in carrying out the experiment arose from variation in the battery and in the resistance balance. From these causes the results of two days' experiments were rejected, as unlikely to repay the trouble of reduction. On the last day (December 3, 1881) the first difficulty was overcome by using three large DANIELL cells (charged with zinc sulphate) in multiple arc. As precautions against rapid change of temperature the copper coils were wrapped thickly round with strips of blanket and deposited in a closed box. The delicacy of our arrangements was such that about $\frac{1}{1000}$ of a degree centigrade would manifest itself, so that it was hopeless to try to maintain the resistance balance absolutely undisturbed. The mode of applying a suitable correction will presently be explained. On December 3, partly by good luck, the necessary correction remained small throughout. In order to avoid a direct action of the current upon the galvanometer needle, the coil was placed at a considerable distance, at the same level, and with its plane horizontal. Any outstanding effect of the kind would, however, be eliminated from the final result by the reversals practised.

The induction throws were always taken by reversal of the battery current. A reversal has two advantages over a simple make or break. In the first place the effect is doubled and is therefore more easily measured; and in the second the battery is more likely to work in a uniform manner, the circuit being always closed except for a fraction of a second at the moment of reversal. The key was of the usual rocker and mercury cup pattern.

The galvanometer was one belonging to the laboratory of about 80 ohms resistance.

It was set up by Mr. GLAZEBROOK for his experiments by an allied method, and with its appurtenances was ready for use at the time that this determination of self-induction was undertaken. The scale was divided into millimetres, and was placed at a distance of 218 centims. from the galvanometer mirror. The instrument was adapted for ballistic work, as the vibrations were subject to a logarithmic decrement of only about ·0142.

The electric balance was provided for by a resistance box from Messrs. ELLIOTTS. The battery current after leaving the reversing key divides itself on entering the box, each part traversing 10 ohms. At the ends of these resistances come the galvanometer electrodes. The first part of the current now traverses the copper coil, and the second part other resistances, after which the two parts reunite and pass back to the battery. In the use of the "other resistances," a special arrangement was adopted which I must now explain. The resistance of the copper coil being somewhat under 24 ohms, the most obvious way to obtain a balance was to add to it a piece of adjustable wire until the whole would balance 24 ohms from the box. The objection to this plan is that the smallest known disturbance which we can then introduce, *i.e.*, by the addition or subtraction of a single unit, is much too great for the purpose.

The difficulty thus arising is completely met by the use of high resistances, taken from a second box, in multiple arc with the 24 ohms.

In order to balance the copper coil and its leading wires at the actual temperature (about 14°), 753 ohms were required in multiple arc with the 24. To calculate the resultant resistance we have

$$\tfrac{1}{24}+\tfrac{1}{753}=\cdot 041666667+\cdot 001328021=\cdot 042994688=\tfrac{1}{23\cdot 25869},$$

so that the resistance of the copper coil in terms of the units of the box is 23·25869. A suitable deflection θ was obtained by the substitution of 853 for 753 in the auxiliary box. In this case

$$\tfrac{1}{24}+\tfrac{1}{853}=\cdot 041666667+\cdot 001172333=\cdot 042839000=\tfrac{1}{23\cdot 34322};$$

so that the additional resistance was

$$\delta Q=\cdot 08453 \text{ unit.}$$

It may be remarked that if the copper coil had been about 1° warmer, its resistance would have been greater by $\frac{1}{300}$th part, and the balance would have required 853 instead of 753 in multiple arc with the 24.

On account of the progressive changes already mentioned, it was advisable to alternate the observations of α and θ as rapidly as possible, and to occupy no more time than was really necessary in taking the readings. A good deal of time may be saved by working the key suitably, and by opening and closing the galvanometer branch (at a mercury cup provided for the purpose) so as to avoid producing unnecessary swings, and to stop those due to induction when done with; but it is

unnecessary to go into detail in this part of the subject. After a little practice two induction throws, starting with opposite directions of the current, and two observations of steady deflection, one in each position of the reversing key, could be made in about seven minutes. The vibrations of the galvanometer needle were damped by the operation of a current in a neighbouring coil, the current being excited by a LECLANCHÉ cell and controlled by a key within reach of the observer at the telescope. The readings were taken by Mrs. SIDGWICK, while I reversed the battery current, shifted the resistances, and recorded the results.

In the simple theory of the method the induction throw is supposed to be taken when the needle is at rest and when the resistance balance is perfect. Instead of waiting to reduce the free swing to insignificance, it was much better to observe its actual amount and to allow for it. The first step is, therefore, to read two successive elongations, and this should be taken as soon as the needle is fairly quiet. The battery current is then reversed, to a signal, as the needle passes the position of equilibrium, and a note made whether the free swing is in the same or in the opposite direction to the induction throw. We have also to bear in mind that the zero about which the vibrations take place is different after reversal from what it was before reversal, in consequence of imperfection in the resistance balance. At the moment after reversal we are therefore to regard the needle as displaced from its position of equilibrium, and as affected with a velocity due jointly to the induction impulse and to the free swing previously existing. If the arc of vibration (*i.e.*, the difference of successive elongations) be a_0 before reversal, the arc due to induction be a, and if b be the difference of zeros, the subsequent vibration is expressed by

$$\tfrac{1}{2}(a \pm a_0)\sin nt + b\cos nt,$$

in which t is measured from the moment of reversal, and the damping is for the present neglected. The actually observed arc of vibration is therefore

$$2\sqrt{\{\tfrac{1}{4}(a \pm a_0)^2 + b^2\}}$$

or with sufficient approximation

$$a \pm a_0 + \frac{2b^2}{a}$$

so that

$$a = \text{observed arc} \mp a_0 - \frac{2b^2}{a}$$

In most cases the correction depending upon b was very small, if not insensible. The "observed arc" was the difference of the readings at the two elongations immediately following the reversal. As a check against mistakes the two next elongations also were observed, but were not used further in the reductions. The needle was then brought nearly to rest, and two elongations observed in the now reversed position of the key, giving with the former ones the data for determining the imperfection of the resistance balance. As the needle next passed the position of equilibrium, it was

acted upon by the induction impulse (in the opposite direction to that observed before), and the four following elongations were read.

These observations of the throw were followed as quickly as possible by observations of the effect of substituting 853 for 753 units in the auxiliary arc. As soon as the vibrations could be reduced to a moderate amplitude, readings of three or four consecutive elongations were taken. The galvanometer contact was then broken, and the battery key reversed. When the needle had swung over to the other side, the galvanometer contact was renewed, and four elongations were observed. The difference between the two positions of equilibrium represented the disturbance of the resistance balance.

The whole of this disturbance, however, was not due to the additional 100 introduced, but required correction for the corresponding effect observed even with 753 units in the auxiliary arc. For this purpose it was only necessary to add or subtract the difference between the equilibrium positions of the needle with the key in the two positions, as deduced from the observations immediately preceding the induction throws; and in order to eliminate the influence of the progressive change, the mean of these differences as found before and *after* the insertion of the extra 100 units was employed. This result was compared with the mean of the four induction throws contiguous to it, two preceding and two following, and in this way a ratio obtained which was independent of the gradual but unavoidable changes in the battery current and in the copper resistance. After about half the readings had been taken the galvanometer connexions were reversed.

A specimen set of observations will now be given.

$3^h\ 36^m$	[753]	L	264·4
		Induction	246·6
$3^h\ 38^m$		R	262·5
$3^h\ 38^m$		Induction	245·9
$3^h\ 40^m$	[853]	R	182·3
$3^h\ 41^m$		L	344·7
$3^h\ 44^m$	[753]	L	264·4
$3^h\ 44^m$		Induction	245·7
$3^h\ 45^m$		R	263·1
		Induction	245·6

At $3^h\ 36^m$ with 753 units in the auxiliary arc and with battery key to the left, the position of equilibrium, as deduced from two elongations, was 264·4 on the galvanometer scale. The arc of vibration due to induction consequent on shifting the key from left to right, corrected for the free swing, but uncorrected for damping, was 246·6. In like manner with key to the right, the equilibrium position at $3^h\ 38^m$ was

262·5 and the arc due to induction was 245·9. The difference 1·9 between 264·4 and 262·5 represented the defect of balance. In the second set of induction throws the corresponding difference is 1·3, showing that the changes of temperature in progress were (at this stage) improving the balance of resistances. The difference between the readings R and L with 853 units is 162·4, the reading L being the higher. Since the reading L is also higher with 753 units, we have to *subtract* from 162·4 the mean of 1·9 and 1·3, *i.e.*, 1·6. The corrected value is thus 160·8. With this we have to compare the mean of 246·6, 245·9, 245·7, 245·6, *i.e.*, 245·9, and we thus obtain as the ratio of the two effects

$$\frac{245{\cdot}9}{160{\cdot}8}=1{\cdot}529$$

The numbers obtained in this way were 1·535, 1·532, 1·529, 1·528, mean 1·5310; and with galvanometer reversed 1·534, 1·529, 1·530, 1·530, 1·532, mean 1·5310. The reversal of the galvanometer appears to have made no difference, and we have as the mean of all 1·5310. The comparison of the partial results shows that during the hour and a half over which the readings extended the battery current fell slowly about one part in 120, and that the resistance of the copper gradually increased, until the balance was perfect, and afterwards became too great, the whole change being about one part in 6000, which would correspond to about one-twentieth of a degree centigrade.

A small correction is required in identifying the above determined ratio with $2\sin\frac{1}{2}\alpha/\tan\theta$. If A be the induction arc and B be difference of equilibrium positions with 853 units when the commutator is reversed,

$$\tan 2\alpha=\frac{\frac{1}{2}\mathrm{A}}{\mathrm{D}},\qquad \tan 2\theta=\frac{\frac{1}{2}\mathrm{B}}{\mathrm{D}}$$

where

D= distance of mirror from scale=218 centims.

From these we get

$$\frac{2\sin\frac{1}{2}\alpha}{\tan\theta}=\frac{\mathrm{A}}{\mathrm{B}}\,\frac{1-\frac{11}{32}\frac{\mathrm{A}^2}{4\mathrm{D}^2}}{1-\frac{1}{4}\frac{\mathrm{B}^2}{4\mathrm{D}^2}}$$

or in the present case with A=24·5, B=16·0,

$$\frac{2\sin\frac{1}{2}\alpha}{\tan\theta}=\frac{\mathrm{A}}{\mathrm{B}}({\cdot}99925)$$

and

$$\frac{\mathrm{A}}{\mathrm{B}}=1{\cdot}5310$$

So far we have omitted to consider the effect of damping, which must necessarily cause the observed value of A to be too small. If λ be the logarithmic decrement, the correcting factor is $(1+\lambda)$. The throw from zero to the first elongation is diminished by the fraction $\frac{1}{2}\lambda$, and the distance from zero to the second elongation is

too small by the fraction $\frac{3}{2}\lambda$. Observations made in the usual manner after the other readings were concluded gave with considerable accuracy

$$\lambda = \cdot 0142$$

The time of vibration was taken simultaneously. It appeared that

$$T = 11 \cdot 693 \text{ seconds}$$

A sufficient approximation to the ratio of currents $x' : x$ can be obtained by neglecting in both cases the current through the galvanometer, whose resistance (80 units) was considerable in comparison with the other resistances. On account of the small resistance of the battery, the difference of potentials at the battery electrodes may be regarded as given. On these suppositions we get at once

$$\frac{x'}{x} = \frac{10 + 23 \cdot 25869}{10 + 23 \cdot 34322}$$

whence

$$\log \frac{x'}{x} = \bar{1} \cdot 99891$$

A more elaborate calculation, in which the finite conductivity of the galvanometer was taken into account, gave a practically identical result

$$\log \frac{x'}{x} = \bar{1} \cdot 99886$$

We may now enter the numbers in the formula

$$L = \delta Q \frac{x'}{x} \cdot \frac{T}{2\pi} \cdot \frac{A}{B} (\cdot 99925)(1 + \lambda)$$

in which we must remember that δQ is to be expressed in absolute measure. Now the value given before, viz.: $\delta Q = \cdot 08453$, is expressed in B.A. units. What this would be in absolute units involves the entire question to whose solution this paper is directed. We will suppose that

$$1 \text{ B.A. unit} = \cdot 987 \text{ ohm}$$

δQ	log $\cdot 08453 \times 10^9$	$= 7 \cdot 92701$
Correction to absolute units . . .	log $\cdot 987$	$= \bar{1} \cdot 99432$
A : B	log $1 \cdot 5310$	$= \cdot 18498$
Correction for finite arcs	log $\cdot 99925$	$= \bar{1} \cdot 99967$
Correction for damping	log $1 \cdot 0142$	$= \cdot 00612$
Time of vibration	log $11 \cdot 693$	$= 1 \cdot 06793$
Ratio of currents	log (x'/x)	$= \bar{1} \cdot 99886$
		$9 \cdot 17889$
	log 2π	$= \cdot 79818$
	log L	$= 8 \cdot 38071$

whence

$$L=2\cdot4028\times10^8 \text{ centims.}$$

The value by *à priori* calculation is

$$L=2\cdot400\times10^8 \text{ centims.}$$

about one part in a thousand lower.*

Correction for level.

If the axis of rotation deviate from the vertical in the plane of the meridian a corresponding correction is required. If I be the angle of dip, and β the deviation of the axis from the vertical towards the north, the electromotive forces are increased in the ratio $(1+\tan I\,\beta):1$, in which proportion we must suppose GK increased. (See pp. 106, 124 of former paper.) The angle of dip at Greenwich for 1881 is about 67° 30′, so that

$$\tan I=2\cdot414$$

The correction for an error in level is thus of the first order, and is magnified by the largeness of the angle of dip in these latitudes. If the experiments were made at the magnetic equator, we should not only reduce the correction for level to the second order, but also obtain the advantage of a nearly doubled horizontal force.

Observations on the level were made by Dr. SCHUSTER on June 1, by myself on August 30, and by Mrs. SIDGWICK on October 13, and on November 11 and 23. The August observations gave $\beta=\cdot26'$; the October observations gave $\beta=\cdot30'$; and the November observations gave $\beta=\cdot25'$. The position of the axis is necessarily to a slight extent indefinite, and the differences are probably accidental. The same level was used throughout, and the value of its graduations was tested. We may take

$$\beta=+\cdot27'=+\cdot000079 \text{ circular measure}$$

and

$$1+\tan I\,\beta=1\cdot00019$$

* A further small correction is called for by the fact that at actual temperature of the room (about 14°) the resistances given by the boxes were not exactly multiples of the B.A. unit. The difference in the case of the principal box, which is marked as correct at 14°·2, may be neglected, but the resistances taken from the auxiliary box (marked 18°·3) must have been smaller than their nominal value, to the extent of a little over one part in a thousand. By the same fraction δQ, and consequently L, must be *greater* than is supposed in the above calculation. The corrected value of L will be

$$L=2\cdot4052\times10^8$$

It is about *two* parts in a thousand greater than that found from the measured dimensions, and is, in my opinion, quite as likely to be correct.

Correction for torsion.

To determine τ, about five complete turns in either direction were given to the upper end of the fibre. The difference of reading for one turn was found to be in June 2·58, and in August 2·45. If we take as the mean 2·51, we get

$$\tau = \frac{2 \cdot 51}{4\pi \times 2670} = \cdot 000075$$

Value of GK *corrected for level and torsion.*

Calling the corrected value $\mathfrak{GK}$, we have

$$\mathfrak{GK} = \frac{\mathrm{GK}(1 + \tan \mathrm{I}\beta)}{1 + \tau} = \frac{1 \cdot 00019}{1 \cdot 000075} \mathrm{GK}$$

so that

$$\log \mathfrak{GK} = 8 \cdot 17686$$

The corrections are in fact almost insensible.

Calculation of U.

In this we take for $\mathfrak{GK}$ the value just found. For L we take the mean of the values found by *à priori* calculation and by direct experiment, *i.e.*,

$$\mathrm{L} = 2 \cdot 4026 \times 10^8.$$

Thus

$$\log \frac{2\mathrm{L}}{\mathfrak{GK}} = \cdot 50485, \qquad \frac{2\mathrm{L}}{\mathfrak{GK}} = 3 \cdot 1978$$

For the values of $\tan \phi$ and $\tan \phi_0$ we must anticipate a little. The ratio is itself in some degree a function of the speed, but it will suffice to take the values applicable to the highest speed, for which $\tan \phi_0 : \tan \phi = 7 \cdot 81 : 439 \cdot 41$.

Thus

$$\frac{\tan \phi}{\tan \phi - \tan \phi_0} = 1 \cdot 0181$$

and

$$\log \mathrm{U} = \cdot 84325$$

Measurement of tan μ.

This is the tangent of the angle through which a suspended magnetic needle would be turned when the principal magnet is presented to it at a distance $\sqrt{(a^2+b'^2)}$ to the east or west, the axis of the principal magnet lying east and west. Actual measurements with the aid of the auxiliary magnetometer were made in April, June, and November; and as a check upon the constancy of the magnetic moment frequent observations were taken of the time of vibration.

To explain the procedure it will be sufficient to take the data of the November measurement. Two positions were chosen for the principal magnet, nearly equidistant from the suspended magnet, to the east and west. The length of the line joining the two positions was 695 millims., and it passed horizontally about 36 millims. below the suspended magnet. In each position the magnet was reversed backwards and forwards several times and readings taken. When the principal magnet was to the east, the mean difference of readings due to reversal was 13·55 divisions on the millimetre scale. When the principal magnet was in the westerly position, the corresponding difference of readings was 14·61. We are to take the mean of these, *i.e.*, 14·08, as the difference of readings due to reversal at a distance of 347·5 millims. The half of this, or 7·04, corresponds to the simple presentation or removal of the magnet. The distance from mirror to scale was 2670 millims, so that the tangent of the angle of deflection was $\frac{7\cdot04}{2\times2670}$. This result has to be adjusted to correspond with the distance $\sqrt{(a^2+b'^2)}$, in place of 347·5. Hence

$$\tan\mu=\frac{(347\cdot5)^3\times7\cdot04}{(238\cdot5)^3\times2\times2670}=\cdot00408$$

In this calculation the error due to the principal magnet having been necessarily placed at a different level from that of the suspended magnet is ignored. As a matter of fact a relatively considerable correction is required. If θ be the altitude of one magnet as seen from the other, the observed effect is too small in the ratio $(1-3\theta^2):1$. The above written value of tan μ requires to be increased about 3 per cent.; so that we take

$$\tan\mu=\cdot00420.$$

Measurement of D.

For the first and second series of spinnings the distance from mirror to scale was measured exactly as described by Dr. SCHUSTER (p. 126 of former paper). The value adopted for the second series, after correction for the thickness of the glass window in the magnet box, was

$$D=2669\cdot0 \text{ millims.}$$

The same method of measurement was applied at the beginning of the third set, and a watch was kept by means of the measuring rods already spoken of (p. 667). Slight movements were in fact observed, principally of the nature of a recovery of the telescope stand from the rather violent treatment to which it had been subjected as a test. Minute corrections are accordingly introduced into the tabular statement (p. 693), so as to make the results of different days comparable. At the close of the spinnings the direct measurement was repeated, when there appeared a slight discrepancy between the results obtained by Mrs. SIDGWICK and myself. It is in fact rather a difficult matter to say exactly when the pointer has advanced to the equilibrium position of the centre of a suspended mirror, which cannot be prevented from swinging. Although the amount in question was not important, I thought it might be more satisfactory to check the result by another method, and therefore arranged a travelling microscope focussed alternately upon the centre of the mirror and upon a scratch on the window of the magnet box, by which the distance between these two points was determined. The remaining distance between the scratch and the scale was easily measured with the rod. The result tended to confirm the smaller value previously found. The value adopted for the spinnings of the third series before November 5 is

2668·8 millims.

and for November 5 and subsequent nights

2669·4 millims.

From these numbers we have to subtract 1·1 millim., as a correction for the thickness of the glass window; so that

D before November 5 $=$ 2667·7 millims.
D November 5 and after $=$ 2668·3 millims.

These distances are expressed in terms of the divisions of the scale, whose exact agreement with millimetres is of no consequence.

Reduction of results.

In order to give a clear idea of the results and of the manner in which they have been reduced, it will be advisable to quote from the note book the details of one set of spinnings. I have chosen at random one of the third series made on October 31, 1881, with a speed of "45 teeth."

TABLE I.

		Time* P.M.	Readings of auxiliary magnetometer in millims.	Readings of principal magnetometer in millims.	Readings corrected by auxiliary magnetometer.	Sum.	Difference.	Mean deflection.	Standard *minus* copper, in bridge-wire divisions.	Temperatures.			Beats per minute.	Correction to middle of bridge.	Correction to 13° of standard.	Correction to 59 beats.	Correction to 17° of fork.	Corrected deflection.
										Standard.	Air.	Fork.						
Contact open.	I.	8.16	81·3	593·38	593·38	1197·24	10·48											
	II.	8.18	81·6	604·16	603·86	1197·27	10·45											
	III.	8.20	82·2	594·31	593·41	1197·51	10·69	5·29										
	IV.	..	83·0	605·80	604·10	1197·55	10·65											
	V.	8.23	83·8	595·95	593·45	1197·50	10·60											
	VI.	8.25	84·2	606·95	604·05													
		8.36	..	..	..	..	..	..	+212	9·95	12·2	12·5	56					
Contact closed.	VII.	8.45	83·1	903·38	901·58	1197·69	605·47											
	VIII.	8.47	82·5	297·31	296·11	1197·65	605·43											
	IX.	8.50	81·4	901·64	901·54	1197·96	605·12	302·56	Mean −52	Mean 10·02	..	Mean 13·05	Mean 56·5	+·03	−·27	−·10	−·12	302·10
	X.	8.52	81·0	296·12	296·42	1197·75	604·91											
	XI.	8.55	81·0	901·03	901·33	1197·89	604·77											
	XII.	8.58	81·1	296·36	296·56													
		9.3	..	..	..	..	..	..	−316·5	10·1	12·0	13·6	57					

* To prevent misapprehension it may be mentioned that the times given in this column are approximate only and were not relied upon to secure simultaneous readings of the two magnetometers. A signal was passed from one observer to the other at the beginning and end of the readings.

The first column gives the number of the spinning, the first six being made with wire circuit open, and the last six with the wire circuit closed. In spinnings I., III., V., VIII., X., XII., the rotation was in the direction reckoned negative, and in the remaining ones positive. The second column gives the time, the third the reading of the auxiliary magnetometer, the fourth the reading of the principal magnetometer, the fifth the result of correcting the latter by the former, the sixth and seventh the approximately constant sums and differences of consecutive pairs of numbers in the fifth column, and the eighth gives the mean deflection from zero, *i.e.*, 5·29 for the open contacts, and 302·56 for the closed contacts.

The ninth column shows the results of the resistance comparisons between the platinum-silver standard and the revolving copper coil before and after the closed contact spinnings. The first number (+212) means that at $8^h\ 36^m$ the resistance of the standard exceeded that of the copper by 212 bridge-wire divisions, each of which represents $\frac{1}{20000}$ of an ohm. It will be seen that during the spinnings the resistance of the copper increased, which accounts for the gradual fall observable in the seventh column. The mean of the comparisons before and after spinning is taken to correspond with the mean deflection 302·56. The three following columns show respectively the temperatures of the water in which the standard was immersed, of the air in the neighbourhood of the copper coil, and of the standard tuning-fork, while the thirteenth column gives the number of beats per minute between the electrically maintained and the standard fork.

For the sake of more convenient comparison of the results obtained at the same speed on different nights, small corrections are calculated to reduce the actually observed deflections in the eighth column to what they would have been in a standard condition of the resistance and of the speed. Under each of these heads we have two corrections to consider. In the first place the copper circuit differed in resistance from the standard coil by the outstanding (−52) divisions of the bridge wire. The resistance of the whole being about 24 ohms, each division of the wire corresponds to one part in 480,000, so that in the present case the correction is additive and equal to 52 parts in 480,000, *i.e.*, is equal to +·03 division of the scale. This is given in the fourteenth column. Secondly, the resistance of the standard itself depends upon a variable temperature. The mean temperature of the standard in this series was about 13°, to which all the observations are reduced. In the present case the temperature was below the normal, so that the resistances were too small and the deflections too large. Accordingly the correction is negative. To estimate its amount the change of resistance with temperature is taken at three parts in 10,000 per degree ; so that in the present case we are to subtract 2·8 parts in 3000 of the whole deflection, *i.e.*, ·27, as entered in the fifteenth column. With use of these corrections we obtain the deflection as it would have been observed had the resistance of the revolving circuit (together with the long copper bars by which it was connected with the bridge) been on every occasion exactly that of the standard at 13°.

In like manner two other very small corrections have to be introduced to make the results correspond exactly to a normal speed of rotation. The standard number of beats is taken at 59, and the standard temperature of the fork at 17°. In the specimen set the number of beats is $2\frac{1}{2}$ per *minute* too small, which means that the octave of the electrically maintained fork made (relatively to the other fork) $2\frac{1}{2}$ complete vibrations per minute *too many*. The whole number of vibrations per minute being 60×127, the speed was too great by $2\frac{1}{2}$ parts in 60×127, by which fraction the observed deflection must be reduced. The correction is thus $-\cdot10$. But besides this the standard fork at 13·05° vibrated faster than its normal rate at 17°, by about one part in 10,000 for each degree of difference. The correction for this is $-\cdot12$.

In addition to the corrections already mentioned the observations of November 5 and after were subjected to another small correction for the observed change in D.

The accompanying Table (II.) exhibits the results of the second series in a manner which after what has been said will not require much explanation. Column VIII. gives in each case what the deflection would have been if the revolving circuit and the copper connecting bars had exactly balanced the platinum-silver standard at 16°, the electric fork vibrating at such a speed as to give 59 beats per minute with the standard fork at 17°, and thus allows us to test the agreement or otherwise of the results obtained on various occasions at the same speed. From this point onwards the means only need be considered; but as there is reason (as already explained) to distrust the observations of August 29, I have added a second mean from which the distrusted elements are excluded. The deflection (d) thus arrived at is equal to $\mathrm{D}\tan 2\phi$, whereas what we require is $2\mathrm{D}\tan\phi$. The connexion between the two quantities is obtained in a moment from the formula

$$2\tan\phi=\tan 2\phi\,(1-\tan^2\phi)$$

by successive approximation. Thus

$$2\tan\phi=\tan 2\phi\,\{1-\tfrac{1}{4}\tan^2 2\phi+\tfrac{1}{8}\tan^4 2\phi\},$$

or

$$2\mathrm{D}\tan\phi=d-\tfrac{1}{4}\frac{d^3}{\mathrm{D}^2}+\tfrac{1}{8}\frac{d^5}{\mathrm{D}^4}.$$

Column X. gives the value of $2\mathrm{D}\tan\phi$, XI. that of $2\mathrm{D}(\tan\phi-\tan\phi_0)$ in the notation of p. 675, and XIII. that of $\log(\tan\phi-\tan\phi_0)$.

For the further calculation we require the value of ω. If f be the frequency of vibration of the electrically maintained fork, F that of the standard at 17°, N the number of teeth,

$$\omega=\frac{4\pi f}{\mathrm{N}},$$

and when the number of beats is 59 per minute,

$$f=\tfrac{1}{2}(\mathrm{F}-\tfrac{59}{60}).$$

TABLE II.—Second series.

Date, August, 1881.	Open contact deflections.	Closed contact deflections.	Correction to middle of bridge wire.	Correction to 16° of standard.	Correction to 59 beats.	Correction to 17° of fork.	Closed contact deflections, corrected to standard resistance and speed.	Correction for scale reading $-d^3/4D^2$ $+d^5/8D^4$	Closed contact deflections corrected for scale reading.	X. *minus* II.		Logarithm of $\tan\phi - \tan\phi_0$.
	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.	XIII.
						60 TEETH.						
15	3·97	228·60	−·15	+·01	·00	·00	228·46	[− ·42				
23	3·87	228·68	−·33	−·01	·00	·00	228·34	+ ·00				
26	3·95	228·41	+·04	+·01	−·03	·00	228·43	=]				
Mean	3·93	..	..	..	..	..	228·41	− ·42	227·99	224·06	D=2669·0 millims.	$\bar{2}$·62298
						45 TEETH.						
19	5·25	302·22	−·33	·00	·00	−·01	301·88	[− ·97				
24	5·27	302·31	−·34	+·01	−·02	−·01	301·95	+ ·01				
27	5·21	302·48	−·50	·00	−·04	−·01	301·93	=]				
Mean	5·24	..	..	..	..	..	301·92	− ·96	300·96	295·72	D=2669·0 millims.	$\bar{2}$·74350
						35 TEETH.						
15	6·68	383·41	+·22	+·03	·00	·00	383·66	[−1·98				
24	6·82	383·90	−·18	+·02	+·02	−·01	383·75	+ ·02				
29	6·81	383·53	−·18	−·01	−·05	−·02	[383·27]	=]				
Mean	6·77	..	..	..	..	..	383·56 or 383·70	−1·96 / −1·96	381·60 or 381·74	374·83 or 374·97	D=2669·0 millims.	$\bar{2}$·84645 or $\bar{2}$·84662
						30 TEETH.						
19	7·86	442·79	−·01	+·02	·00	·00	442·80	[−3·04				
26	7·80	442·73	−·18	+·01	−·03	·00	442·53	+ ·04				
29	7·81	441·85	+·20	+·02	−·03	−·01	[442·03]	=]				
Mean	7·82	..	..	..	..	..	442·45 or 442·66	−3·00 / −3·00	439·45 or 439·66	431·63 or 431·84	D=2669·0 millims.	$\bar{2}$·90773 or $\bar{2}$·90794

For F at 17° we take 128·130 (see p. 137 of former paper) so that

$$f=63{\cdot}574.$$

Thus

$$\log(2\pi . f . \mathfrak{G}\mathfrak{K})=10{\cdot}77832=\log(10^{10}\times 6{\cdot}0023),$$

and

$$R=\frac{10^{10}\times 6{\cdot}0023}{N(\tan\phi-\tan\phi_0)}\left[\tfrac{1}{2}(1+{\cdot}00422\sec\phi)+\sqrt{\{\tfrac{1}{4}(1+{\cdot}00422\sec\phi)^2-U(\tan\phi-\tan\phi_0)^2\}}\right]$$

in which

$$\log U={\cdot}84325$$

TABLE III.—Second series.

	Number of teeth.			
	60.	45.	35.	30.
R by preceding formula in ohms. .	23·639	23·655	23·660 23·651	23·670 23·659
Resistance of standard at 16°. . .	23·642	23·658	23·663 23·654	23·673 23·662
Resistance of standard at 13°. . .	23·621	23·637	23·642 23·633	23·652 23·641

The immediate result of the formula is the resistance in absolute measure of the revolving circuit, on the supposition that with the connecting bars it exactly balances the standard at 16°. The resistance of the standard itself is therefore given by addition of the resistance of the bars, *i.e.*, ·003 ohm. In the last line the results are reduced to the temperature of 13° for comparison with the third series.

TABLE IV.—Third series.

Date, 1881.	Open contact deflections.	Closed contact deflections.	Correction to middle of bridge wire.	Correction to 13° of standard.	Correction to 59 beats.	Correction to 17° of fork.	Correction to D = 2667·7.	Closed contact deflections corrected.	Correction for scale reading as in Series II.	Correction for curvature of scale.	Closed contact deflections corrected for scale.	XII. *minus* II.		Logarithm of tan ϕ − tan ϕ_0.
I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.	XIII.	XIV.	XV.
							60 TEETH.							
Oct. 22	3·92	228·47	+·19	−·06	−·06	−·07	..	228·47						
Oct. 29	3·95	228·74	−·08	−·10	−·06	−·08	..	228·42						
Nov. 7	3·92	228·21	+·27	+·13	−·06	−·01	−·05	228·49						
Mean	3·93	..	..	..	..	..	..	228·46	−·42	+·05	228·09	224·16	D=2667·7 millims.	$\bar{2}$·62339
							45 TEETH.							
Oct. 29	5·20	302·93	−·37	−·20	−·08	−·11	..	302·17						
Oct. 31	5·29	302·56	+·03	−·27	−·10	−·12	..	302·10						
Nov. 5	5·22	301·84	+·31	+·17	−·11	−·02	−·07	302·12						
Mean	5·24	..	..	..	..	..	..	302·13	−·96	+·08	301·25	296·01	D=2667·7 millims.	$\bar{2}$·74414
							35 TEETH.							
Oct. 22	6·77	383·49	+·64	−·16	−·05	−·12	..	383·80						
Oct. 31	6·77	383·92	+·39	−·33	−·10	−·15	..	383·73						
Nov. 7	6·73	383·48	+·29	+·18	−·10	−·03	−·09	383·73						
Mean	6·76	..	..	..	..	..	..	383·75	−1·96	+·11	381·90	375·14	D=2667·7 millims.	$\bar{2}$·84702
							30 TEETH.							
Oct. 29	7·92	443·64	−·25	−·26	−·12	−·17	..	442·84						
Nov. 5	..	442·31	+·54	+·22	−·17	−·03	−·10	442·77						
Nov. 7	7·90	..	..	..	..	..	..	..						
Nov. 10	7·94	442·74	+·16	+·19	−·11	−·04	−·10	442·84						
Mean	7·92	..	..	..	..	..	..	442·82	−3·00	+·11	439·93	432·01	D=2667·7 millims.	$\bar{2}$·90830

TABLE V.—Third series.

	Number of teeth.			
	60	45	35	30
R by formula	23·616	23·618	23·627	23·635
Resistance of standard at 13° .	23·619	23·621	23·630	23·638

If we compare the results of the second and third series at the same speed, we find the agreement satisfactory (with a partial exception at the speed corresponding to 45 teeth), especially if we take the means from which the observations of August 29 in the second series are excluded. Adding together all the results of each series we should obtain from the second series 23·638, or with exclusion of August 29, 23·633, and from the third series 23·627, between which the extreme difference is less than one part in 2000. When, however, we compare the values obtained from observations at different speeds, we see from both series, but more especially from the third, evident signs of a tendency to rise with the speed, as if the self-induction of the revolving circuit had been underestimated. In view of the remarkable concordance of the results obtained at the same speed on different nights, it is impossible to attribute these discrepancies to errors of observation, and it is important to consider what cause of systematic disturbance can have remained unallowed for. The first question which presents itself is whether it is possible to admit an error in the adopted value of L sufficient to explain the progression. The proportional correction for self-induction is approximately $-\mathrm{U} \tan^2 \phi$, or for the speed of 30 teeth ·0457. For the speed of 60 teeth the correction will be only one-fourth of this. To bring the results for the two speeds into agreement it would be necessary to increase the value of U by nearly 3 per cent., which would correspond to an increase of about one per cent. in L. It is difficult to believe that the value of L adopted for the wire circuit can be in error to this extent.

Another direction in which an explanation might be looked for would be the influence of air disturbance, or from tremor. The accompanying table, however, shows such an extraordinary agreement of the open contact deflections, both among themselves and with numbers proportional to the speeds of rotation, as to prevent us from supposing that this cause of disturbance can have operated.

TABLE VI.—Deflections with wire circuit open.

	Number of teeth.			
	60	45	35	30
Mean of first series	3·89	5·22	6·74	7·85
,, second series . . .	3·93	5·24	6·77	7·82
,, third series	3·93	5·24	6·76	7·92
After the wire had been removed, December 7 . . .	..	5·23	6·76	..
Numbers proportional to speed	3·94	5·25	6·76	7·88

On the whole, it would appear to be the most probable explanation that there were currents in the ring flowing in circuits not conjugate to the wire circuit, and therefore influencing the induction phenomena. But whatever view we may take on this matter, there is no reason to doubt that the true value will be obtained by introducing such a correction proportional to the square of the speed as will harmonise the several results, a course equivalent to determining the coefficient of self-induction from the spinnings themselves. In this way the numbers corresponding to any two speeds may be made arbitrarily to agree, but the numbers for the two remaining speeds will afford a test of the admissibility of this procedure. The only hypothesis upon which the simple mean of the numbers already obtained for the various speeds should be taken as final would appear to be one that would attribute to the discrepancies an accidental character, and seems quite out of the question.

The simplest way to carry out the correction will be to determine the amount of the coefficient from the two extreme speeds. The squares of the speeds are as

$$1 : \tfrac{16}{9} : \tfrac{144}{49} : 4;$$

so that the difference of the numbers for the two extreme speeds, $23{\cdot}638 - 23{\cdot}619$, *i.e.*, ·019, is three times the quantity by which the lowest is to be reduced. We are accordingly to subtract respectively

$$\cdot0063,\ \tfrac{16}{9}\times\cdot0063,\ \tfrac{144}{49}\times\cdot0063,\ 4\times\cdot0063$$

with the following results.

TABLE VII.—Third series.

	Number of teeth.				Mean.
	60.	45.	35.	30.	
Resistance of standard at 13°, uncorrected	23·619	23·621	23·630	23·638	23·627
Correction proportional to square of speed	·006	·011	·018	·025	..
Resistance of standard at 13°, corrected .	23·613	23·610	23·612	23·613	23·612

It will be seen that the agreement is practically perfect, the coefficient given by the extreme speeds suiting also the requirements of the intermediate speeds. The maximum difference corresponds to about $\frac{3}{100}$ths of a millimetre only in the deflections of the principal magnetometer. The number $23{\cdot}612 \times 10^9$ is therefore to be regarded as the resistance in absolute C.G.S. measure of the platinum-silver standard at 13°. If, however, the correction be rejected, the result will be different by decidedly less than one part in a thousand.

Although the experiments of the second series will not bear comparison with those of the third, it may be well to mention that they lead to substantially the same conclusion. The simple mean (taken with exclusion of August 29) of all the values is 23·633, and after introduction of the correction proportional to the square of the speed, 23·618.

The results of the first series of spinnings are given in Table VIII. They have been reduced by Dr. SCHUSTER, so as to show the value of the platinum-silver standard in absolute measure from the observations of each night at each speed. The mean radius of the coil was taken from the first measurements, and a somewhat higher value of U was employed than the subsequent calculation of the ring currents seemed to justify.

TABLE VIII.—First series.

Teeth.	Resistance in absolute measure of standard at 13°.				
80	23·651,	23·632,	23·628	..	Mean 23·637
60	23·646,	23·629,	23·601	..	Mean 23·625
45	23·678,	23·691,	23·686	..	Mean 23·685
35	23·608,	23·615,	23·632,	23·665	Mean 23·630
30	23·644,	23·639,	23·628	..	Mean 23·637
					23·643

Comparison with the standard B.A. units.

Four distinct sets of comparisons between the platinum-silver standard and the ultimate B.A. units have been effected in the course of these investigations, and two distinct methods have been followed. In the first method two coils of about five units, called for brevity [5]'s, were compared separately with five standard units combined in series with mercury cups. Secondly, the two [5]'s in series were compared with a [10]. Thirdly, the [10], the two [5]'s, and four singles were combined in series and compared with the platinum-silver standard [24]. The differences in every case were expressed in divisions of the wire of FLEMING'S bridge, whose value in terms of the B.A. unit is known. This method is simple enough in principle, but the arrangement of so many mercury connexions is troublesome, and the calculation of the innumerable temperature corrections necessary is tedious. The labour would have been greater still had we not been able to avail ourselves of the previous work of Professor FLEMING, who had carefully compared the various standard units, and had drawn up a chart on which is exhibited the comparative resistances of the coils over a considerable range of tempera-

ture. The mean B.A. unit, in terms of which our results are expressed, was defined by him, but the differences between the single standards is scarcely of importance for our purpose. In calculating the temperature corrections for the two [5]'s, the [10], and the [24], which were all of platinum-silver wire, the co-efficient ·0003 per degree has been used. The temperatures were those of the water in which the coils were immersed. They never differed much from the temperature of the room, and were referred to a Kew standard. The results of three comparisons, executed by Mrs. Sidgwick, are as follows :—

Resistance in mean B.A. units of platinum-silver standard at 13°.

July, 1881	23·9326
September, 1881	23·9341
November, 1881	23·9348

In February, 1882, a fourth determination was executed by myself, in which a different method was employed. Five coils approximately equal to each other and to five units were arranged in a closed case upon a tube of brass. The ten copper terminals emerged below from the ebonite bottom of the case, and rested in mercury cups upon an ebonite base-board, which was so arranged that by a single motion the terminals could be transferred from one set of cups which combined the coils in series to another set which combined them in multiple arc.* In this way resistances are obtained in the proportion of 25 : 1, independently of any exact equality between the single coils ; for it is obvious that if the resistance in series is given, the resistance in multiple arc is a maximum in the case of equality, and therefore varies little, even if the equality be not exact. By the aid of this apparatus the [24] was compared with a standard unit, without the assistance of other coils. In the first place [24]+[1] was compared with the five coils in series, and in the second place the [1] was compared with the five coils in multiple arc. The only precaution necessary is to effect the second comparison so quickly after the first that the five coils have no time to change their temperature. Two determinations by this method on different days gave as the resistance of [24] at 13°

23·9350, 23·9358—mean, 23·9354.

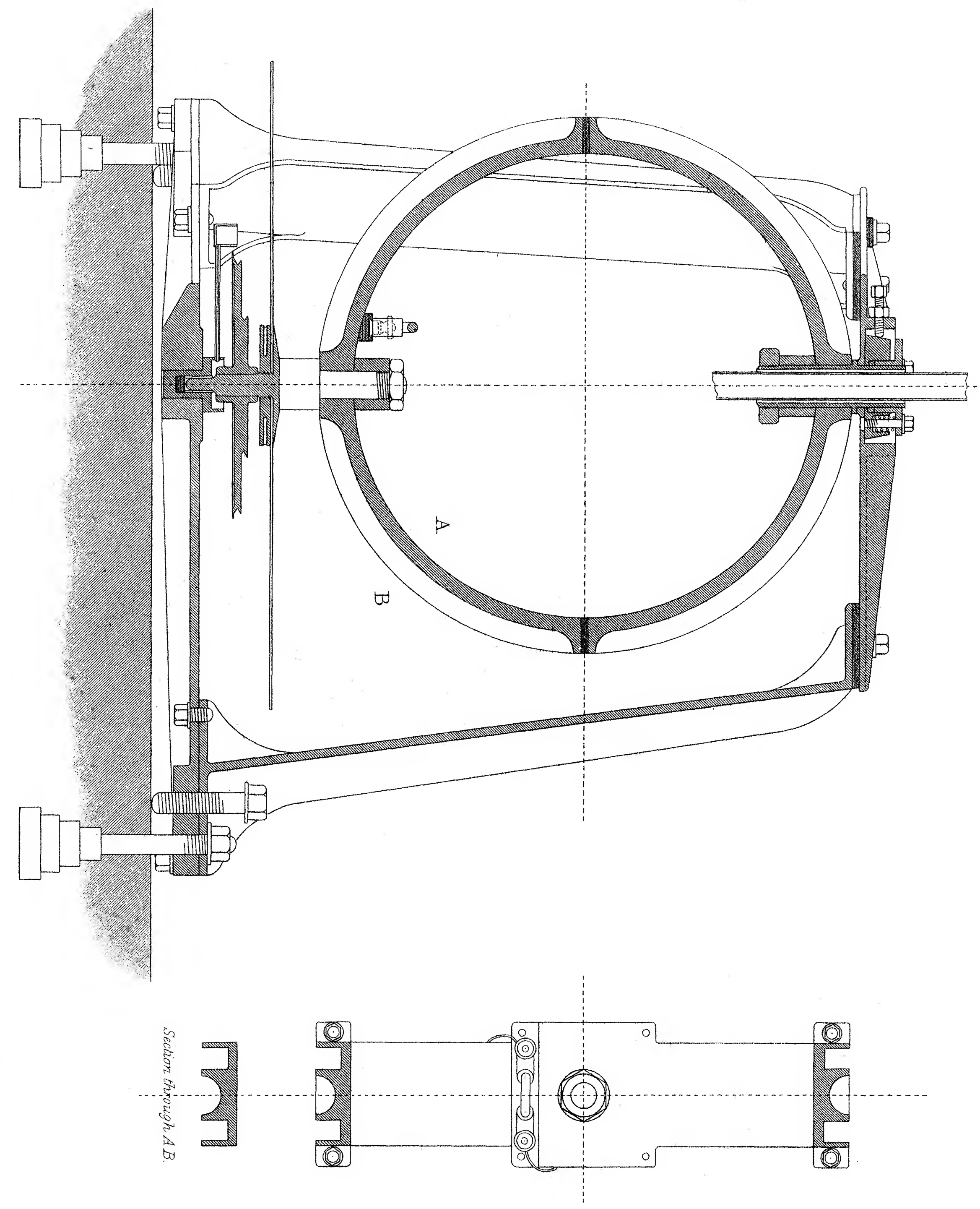
It would seem not impossible that the resistance of [24] has gradually increased, but the changes are unimportant. We will take as the resistance with which the absolute measurement is to be combined, that found in November 23·9348 ; so that

$$23{\cdot}9348 \text{ B.A. units} = 23{\cdot}612 \times 10^9 \text{ C.G.S.} = 23{\cdot}612 \, \frac{\text{earth quadrant}}{\text{second}}$$

Hence, as the result of the investigation, we conclude that

$$1 \text{ B.A. unit} = {\cdot}98651 \, \frac{\text{earth quadrant}}{\text{second.}}$$

* I believe that Professor Rowland has used a contrivance of this sort.



Scale one sixth

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